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# TAXONOMY AND EVOLUTION

By X.

"Some passages in this book, if taken alone and read hastily, may appear to discourage systematic Zoology. This is far from my intention. No one can study the great naturalists of the seventeenth and eighteenth centuries without feeling how seriously their work is impaired by the defective systems of the time. It is not systematic but aimless work that I deprecate—work that springs from no real curiosity in Nature and attempts to answer no scientific questions."—L. C. Miall, "Natural History of Aquatic Insects," Preface, p. i.

## INTRODUCTION

LINNÆUS bestowing Latin names upon animals and plants was simply tripping gaily across the back of a half submerged Behemoth and mistaking it for dry land. Now the beast is careering around, and in spite of zoological congresses and international rules nobody quite knows what to do with him. No doubt when some zoological czar arises and issues his fiat a uniform system of nomenclature will be adopted and things will begin to straighten themselves out. This can only be a matter of time—the past can not be altered. On systematists to-day necessarily devolves the dull, difficult and important duty of going through the descriptive work of the early naturalists and emending it; so that Spallanzani's derisive sobriquet of "nomenclature naturalists" was a little unjust, even in his time.

Whatever opinions may be held upon the genius of Linnæus, in justice to him it should be said that it was not until his example had been followed by a crowd of other workers eager to attain to immortality by way of the back door he had left open that the fat was really in the fire.

Well knowing the confusion into which systematic work in zoology was brought by the early naturalists, modern systematists in our opinion will be the authors of a similar confusion in the future if some of the slipshod methods of modern systematics are not corrected. Moreover, a confused nomenclature is not the least of the evils which second-rate systematic work brings in its train.

Systematists with a proud curl of the lip may tell us that the work is not done now as it once was. Indeed, to those who are not able to project themselves into the future it may seem incredible that the systematists of a later date will be able to find much room for complaint in the elaborate descriptions and careful figures of modern descriptive writers. For the moment, however, it suffices us to point the parable by remarking that in 1780 Spallanzani was able to refer to the "beautiful figures" and "careful descriptions" of a systematic worker on frogs. We, of course, know without seeing them that the figures were not beautiful nor the description, careful—any way in the sense of being complete. We have therefore to reflect whether the zoologists of a future generation will find the work of to-day any freer of faults than that of the past centuries.

#### SYSTEMATIC WORK. GENERAL CONSIDERATIONS

It is necessary to insist at once that systematic work is not merely a question of nomenclature, names and novelties. Systematists have only themselves to thank if such a narrow conception of their province is very widely spread, especially among morphologists and anatomists, who are ready to belittle the value of the systematists' work. But science is measurement and zoology—if you like—is description, and it is impossible to dispense with the systematists' descriptive work. But we think it possible to dispense with a good deal of stuff after this fashion:

Metopidium high, suprahumeral rather long, acute, arcuate and curved at the tips. Pronotum roughly punctured at the bottom of fine furrows. Color dark-ochreous. Posterior horn uniformly cylindrical, undulating or sinuous without rugosities. Underside, scutellum and legs sordid-ochreous.

The phrase "sordid ochreous" comes ready to hand and makes it unnecessary for us to go in search of a suitable comment.

"This is the 30th memoir" writes a systematist "on the Zonitidæ which I have published in this journal, describing in all about 560 new species." We feel inclined to put our hands resolutely on his shoulders and inquire if he ever saw a *ctenophor* swimming in the sea or watched the progress of an *Asterias* towards its prey.

No one can look unmoved upon the Hymenopteran or Helicoid specialist with head bent over a drawer full of shells or dried insects on pins. It is not that we resent concentration or enthusiasm or even specialization, but the systematist has lost touch with his own science of zoology.

Zoology, a cornucopia of marvels, lies at his elbow full to overflowing, but he is unmindful of it. It is as if a man should use the Parthenon only as a convenient place on which to strike a match for his pipe.

The divorce between systematic work and the rest of zoology is the more regrettable because it is practically complete. It is, we admit, expedient that zoology should be divided up into anatomy, morphology and so on. But such a division is allowable only when it is expedient, while for intellectual purposes such a division is and has always been a danger. To obtain facts one must be an analyst, to consider them one must be a synthesist. Between the two there is all the difference between a hodman and a natural philosopher.

But our contention is that not even the plea of practical expediency can justify the extreme state of specialization into which systematic zoology has fallen, making itself manifest in the concatenation of such purely artificial characters as that "the third joint of the antenna is longer than the second, that the mesoscutellum is ovate and the color pink with blue spots." All this simply makes one yawn, though there is this much to be said in favor of this stamp of systematist, that nothing bores him so much as the recitation of one of his own diagnoses or being introduced to the systematist of another group.

Systematic work is a withered branch of the biological tree which there is still hope of rejuvenating. Treviranus long ago remarked that if we once regarded systematic work as a part of biology and nomenclature as a means to an end rather than as an end in itself, both might take their places in science. Let us take every precaution against systematic work becoming one of those unproductive and artificial pursuits which spring up like mushrooms around centers of splendid endeavor and high achievement. After Shakespeare came his commentators. Shall it be said that after biology came the systematists?

We assume that the principal object of systematic work is to discover the *phylo-genetic classification* of animals, for which it is surely necessary that every animal as it passes through the

systematists' hands should be, as far as possible, thoroughly examined and described, no dependence being placed upon a few superficial characters usually selected from the external parts? That the systematist should concern himself, as he does, with the external parts, leaving the anatomy to other workers, we consider is as bad for the systematist himself as it is bad for the science; for himself, he is doing work which can only keep his soul alive with difficulty—superficial clerical work which can be “prompted by no real curiosity and attempts to answer no scientific questions,” and the results of the work itself is often invalidated by the arrival of the destroying angel in the person of the anatomist. For a superficial description often means a wrong classification; whence it follows that any zoo-geographical deductions therefrom are invalidated; while a careless description usually ignores the possibilities of variation and shows no evidence of pains having been taken to make identification easy.

Systematic work, then, is concerned with classification, geographical distribution, variation and identification, and there would be no need for this paper, if it were more generally realized that one thorough examination and description of the whole animal assists those branches of the inquiry more than twenty loose and superficial ones.

Of course systematic workers are not the only zoologists who over-publish; yet they especially might cultivate a little of the salutary reticence of C. L. Nitsch and Alfred Newton, who, with no discredit to themselves, wrote and published little, yet it must be admitted by those with an eye on the extravagant output of others, to the advantage of zoology. The words “*res non-verba*” were the motto of Delle Chiaje, who, like Nitzsch, on his death left behind many important discoveries unpublished and only indicated in his drawings.

#### CLASSIFICATION IN GENERAL

The coming of Evolution meant for systematic workers that no system of classification would henceforth be considered as a serious contribution to science, which was not constructed on phylogenetic lines. It meant the final overthrow of such ideas as Agassiz held, that the divisions of the animal kingdom were instituted by the Divine Intelligence as categories of his mode of thought—of such fantastic systems as those of Rafinesque and Swainson and such strictly artificial ones as the arbitrary ar-

rangements of convenience which should be now used only in those groups where, and for as long as, our knowledge of the anatomy is so slight that some sort of temporary device for sorting out genera and species has to be adopted.

The ideal system is now phylogenetic, *i. e.*, it aims at reconstructing in a genealogical tree the actual lines of descent.

Only those who have attempted the reconstruction of phylogenetic trees understand the intrinsic difficulties of the work. There can be no doubt that the coming of Evolution has put before the systematist a very difficult task. As to whether the methods usually employed by him are adequate to the demands placed upon them we are frankly sceptical.

Fortunately for the systematist the main lines of classification in most groups are given him ready made by the morphologists who have laid down the foundations trusting to the "systematist" to fill in the details. Such classifications—the main phyla, classes and orders are of permanent value, because they are founded upon a combination of characters of tried worth judiciously selected after a careful survey of extensive embryological and anatomical data.

#### SINGLE CHARACTER CLASSIFICATION

On the other hand the minor systems—the families, genera and species—the realm of the "systematist"—too frequently consist of haphazard combination of a few characters selected because of their convenience in not entailing any anatomical work, or selected on account of the ignorance existing of any other—particularly internal—important characters. Ignorance of their morphology has been the main reason for the difficulty in classifying the Coleoptera. Entomologists are especially prone to give their whole attention to what is visible without the aid of dissection. In the Polyzoa the majority of forms are only known by their external appearance and their classification is proportionally unsatisfactory. In the Mollusca reliance is placed on the shell; in mammals the skull and the skin, in birds the plumage are the articles of faith.

Single character classification or diagnosis by one or two characters, as zoological history shows, has proved inadequate—that it is unphilosophical is patent to all.

Such single character classification even when practised by

the great morphologists, men who, being acquainted with the whole of the anatomy of the forms they were classifying, deliberately selected one or two characters after a survey of the whole—was rarely a success. Huxley set out unabashed to classify birds by their palate, and Agassiz fish by their scales—systems which have now shared the fate of most others which set out to erect a classification on the modifications of a single organ alone. Alfred Newton said that there was no part of a bird's organization that by a proper study would not help to settle the great question of its affinities.

The systematist who deals with the minor subdivisions of the animal kingdom—families and genera—should be as much a morphologist as the one who deals with the larger—the phyla and classes.

#### DESCRIPTION

We have pointed out above that the adequacy of a system of classification depends in great measure upon the thoroughness of the description of the species and genera. Classification in all groups has progressed in just proportion to the more exact examination of the species considered in the classification.

The history of zoological research brings out this fact very clearly, beginning with the work of Linnæus, the originator of the superficial diagnosis, passing on through Cuvier, who appreciated the value of anatomical knowledge, to Von Baer, who emphasized the importance of embryology.

It was not a "systematist" as we know him who first correctly classified *Lepas*—the conchologists blindly accepted it as a Mollusc. It was not a "systematist" who first established *Peripatus* as an Arthropod, for the first describer of that animal regarded it as a slug!

How rare it is to find in a description of a new species anything more than an indication of the external parts. It is a peculiarly arbitrary limit to a man's curiosity that restricts his enquiry to the superficial aspect of an animal. A natural philosopher ought never to be satisfied with the external appearance of things. The wisdom of the ancients bids us "beware of what things appear"; and the method of our modern science is one of close and detailed observations. In scattering names broadcast with liberal largesse upon species, varieties and

genera, systematists have sometimes dropped into some curious errors. Teratological specimens have been described as new species and most zoologists have heard of the man who described as a new species the longicorn beetle, the head of which having fallen off, had been fixed on upside down. His examination of a new species makes so slight an impression on his mind that sometimes the same worker has described the same form twice under different names.

The descriptive papers on Mollusca usually consist of short descriptions of the shells, even written in a dead language. This is conchology. Conchologists confine themselves to the patterns and shapes of shells—nature's medallions—numismatics! Much of this work—along with similar productions in entomology and carcinology—we regard as positively flagitious.

Sir Ray Lankester in the article "Zoology" in the *Encyclopædia Britannica* (ed. XI.) remarks that museum naturalists must give attention to the inside as well as to the outside of animals and that to-day no one considers a study of an animal's form of any value which does not include internal structure, histology and embryology in its scope. Agassiz, too in his famous "Essay on classification" wrote that "the mere indication of a species is a poor addition to our knowledge when compared with such monographs as Lyonnet's *Cossus*, Bojanus' 'Turtle' Strauss Durekheim's *Melolontha* and Owen's *Nautilus*."

"But," it will immediately be asked in chorus, "do you seriously suggest that a monographic volume should be devoted to every new species?"

This is a leading question which brings us to the crux of the whole matter, and can not be answered in simple "Yea" or "Nay."

#### THE PROVISIONAL DIAGNOSIS

The amount of analytical study that may be given to any one animal form in any one stage of its development is infinite. The result is that in describing a new species for the purposes of exact phylogenetic classification there must be a limit beyond which it is unnecessary to go. Such a limit can not be otherwise than arbitrarily selected according to the best judgment of the systematic worker as to how much analysis is required to place his new species, although at present, *miserabile dictu*, relatively



very few animals have been thoroughly explored, yet in the distant future, in the millennium, it can not be doubted that every genus, even every species will have been examined *in toto* in every stage of its development and life-history as thoroughly as our instruments and eyesight will allow, and perhaps a whole volume or several volumes will be devoted to every animal form. At present, however, it is a waste of ink to consider a future so far away. A more pressing duty is to consider how far modern methods of superficial diagnosis fulfil the obligations placed upon systematists not to give an exhaustive analysis of animal forms, but to give sufficient data to meet the searching demands of phylogenetic classification.

We are aware of the fact that the convinced and determined systematist does not maintain that the method of superficial diagnosis does meet or is intended to meet the demands we have been indicating. If he reads as far as this and does not throw aside this paper in contempt, he is ready with eager forefinger and glib apology to convict us of begging the question that systematic zoology can be ever anything, or should be ever anything more than we have said.

It is often argued that the superficial diagnosis of the systematic worker is simply a provisional diagnosis awaiting the confirmation of the anatomist. A plausible defence of the provisional diagnosis is advanced by many workers in perfect good faith which it is now necessary to anticipate and examine.

This argument defends the provisional diagnosis on two grounds: (1) The advertisement theory; (2) the recognition mark theory.

The supporters of these theories admit that the provisional diagnosis in no way settles either an animal's systematic position or its validity as a species. But it is alleged to be of value and should be encouraged because it advertises the existence of a presumptive new form which would otherwise remain unknown and overlooked in the store rooms of the museum and laboratory, and because in giving an account of the external parts, at all events, the systematist is describing those features by which we are more or less easily able by a superficial examination to recognize summarily the form when it turns up again.

The first part of our answer amounts to a recapitulation of what has been previously stated in general, viz., that systematics have lost touch with the rest of the science. The output of systematic

work and the output of anatomical and morphological work nowadays move along completely different channels. The work turned out by the systematic worker is scarcely, if ever, conceived in the light of modern biological theory, is rarely couched in terms of modern biology and rarely indicates a problem to be solved or a question to be answered. It proposes distinctions the anatomist sweeps away and hazards affinities the morphologist laughs at. It performs work that has to be done over again, and instead of giving the morphologist what it claims to give him—a sketch map of the country he is to traverse—all it does is to bewilder him with a Will-of-the-Wisp's lantern, an intolerable multitude of slipshod and untrustworthy directions that he has come instinctively to suspect. We can not too often ask the question, why should the work be done twice? Surely it is time that something were done to stop this tremendous rush for publishing provisional diagnoses that more time could be devoted to the systematic study of animal forms, obtaining thereby sound phylogenetic classification, sound deductions in geographical distribution, valid species and a less confused nomenclature.

Thus the systematist's protest that at least he "advertises" presumptive new forms we can reply that he may do so, but that for any purpose other than a dull census of the animal kingdom with a very generous " $\pm$ " to it, he is a positive Benedick of zoologists, for "nobody marks him."

The upholders of the provisional diagnosis will say that at any rate they are giving us a description of the external parts and are increasing our knowledge by so much. True, but by so inconsiderable an amount that when the anatomist comes along with his scalpel he so quickly disposes of the external parts merely by the use of his eyes that it is a matter of indifference whether the former have been described or not. Moreover, the great majority of the tens of thousands of descriptions that are issuing from the press are of animals so closely related to previously described species that such descriptions really amount to little more than a recitation of their distinguishing characters.

It is certainly useful to know that *Caccabis rufa* is to be distinguished from *Perdix cinerea* by its red legs and that the Leporidæ can be discriminated by the character of their upper incisors. But the question may well be asked, what is the use of being able to distinguish one species from another without

being able to record at the same time anything about its bionomics or anatomy which would give the distinction its real value. A great deal is known about the partridges and hares, hence the distinctions alluded to above are useful as an easy way of quickly identifying them. But so long as nothing is known about either of two species that are distinguished we are none the worse off, if both remain indistinguishable.

Finally we would point out that of all people the systematist should know that at present of the forms he advertises and describes so copiously and summarily only a fractional part is, or can be, dealt with by the laboratory worker. We are speaking now of the anatomy pure and simple of new species and genera. The laboratory worker proceeds slowly, is fewer in numbers and has other problems—embryology (descriptive and experimental), heredity, physiology (descriptive and experimental) and morphology to attend to besides purely descriptive anatomy. And yet anatomy—the very corner stone of the temple of zoology—has to be restricted in output because none of the systematists will learn how to use a scalpel or look down a dissecting-microscope—feats in themselves perfectly easy and calling for no special training or faculties.

Possibly the upholders of the provisional diagnosis will maintain that by publishing his account of the difference between closely allied forms the systematist is providing the biologist with a stimulus to discover how much deeper such differences go. But surely it is a strange perversion of a man's natural instinct of curiosity that enables the systematist to rest content with advertising problems instead of endeavoring to equip himself for the task of undertaking them himself, who is eminently suited to the work and whose occupation daily brings him into close contact with them.

Finally we would point out that the enormous mass of species which have been created upon superficial diagnosis so far have remained unincorporated for the most part in the structure it is designed to build up, viz., a clear comprehension of the phylogeny of the lesser divisions of the animal kingdom. It is as though a man were to set about building a house by making a vast quantity of bad bricks and then to leave them scattered about his site in the hopes that some one would come along and make a house of them. Surely it is an economy of effort for the systematist to take up the bricks and build himself, what time

the embryologist and morphologist are engaged upon their own special tasks.

#### THE COMPARATIVE VALUE OF INTERNAL AND EXTERNAL PARTS

Briefly reviewing the discussion as far as we have carried it, it will be seen that we are asking for sound phylogenetic classification of the smaller groups as well as of the larger ones, based not upon single characters, but upon the whole of the characters regarded collectively, for more careful and more thorough morphological methods in description and for the discontinuation of the provisional diagnosis. In view of the desirability of working up sounder schemes of classification from the enormous, unwieldy and superficially known mass of genera and species systematists can be rendering little service by continuing to turn out indiscriminate provisional diagnoses.

It remains now to discuss in greater detail the proposal we bring forward in the place of the provisional diagnosis.

The commonly accepted opinion is that while for the classification of families and orders the internal parts must be taken into consideration, for that of species and genera a summary of the external parts is all that is required. On account of the labor and difficulty sometimes involved in dissection we are too ready to assume that the internal parts in genera and species present a dismal monotony of character which it would be profitless to investigate for systematic purposes.

If it is admitted that internal characters are of value among the higher divisions of the animal kingdom, can the systematist tell us at what precise point in the downward scale they cease to have value, and at which reference need only be made to the external parts? Even supposing for a moment that there is such a limit, we are strongly of opinion that it does not come before the genera.

A genus is of different value in different groups but as a rule it presents so much difference in external form from other genera as to warrant the inference that internal differences of a like extent will be found if sought for. At the present moment a genus is a perfectly arbitrary collection of species. We venture to prophesy that with more elaborate descriptions inter-generic relationships will be more carefully defined and genera will become less heterogeneous and more natural. But this is by the way.

*A priori* it seems improbable that less variety will be found among the various internal systems of organs than in the integumentary or exoskeletal parts. But an argument may be put forward that the external parts in immediate contact with the environmental forces would be the first to register change in the modification of a species. The internal parts as stanchions and bulwarks remain firm to give characters to orders and families, while change makes assault without and gives characters for species. For example, among the Asteroids it is said that the internal organization is so uniform that the only method of classification is to take the different ways in which the demands of the external environment have been met.

But generally speaking a species depends for its survival not simply upon the external front it presents to its environment. An animal's form cannot arbitrarily be divided into external and internal parts. It is an integral whole, and variation and selection may occur anywhere, while the correlation of variation is a text-book commonplace. As opposed to correlative variation there is the law of the independent variation of parts. Not only may variation occurring in one part cause a variation to take place in another, but variation may take place independently in some areas and be limited in another, so that in deciding upon the comparative value of the internal and external parts in any group consideration must be given to both these laws. In the Asteroids, we assume that anatomists have taken the matter in hand and found that the external parts vary as a rule independently of the internal which remain constant. But in how few groups has such a precaution been taken! Is it not rather the general rule simply to *assume* that the internal parts lack variation and are of no value systematically, as, for instance, in the Lepidoptera, where the Lepidopterists expect that a classification based upon the wing-markings or upon wing-neuration can express the true relationship of the various units?

Even in those groups where systematists have dissected and found the internal parts valueless it still remains necessary, in view of the law of independent and unexpected variation of parts for them, to apply the scalpel to every new form.

It is impossible to deny that the external parts are often of extreme systematic importance—they are exposed to the light and develop color patterns (although color is usually an unsafe guide if taken alone), and the external parts of such forms as

Arthropods and Molluscs being hard provide systematists with a sculpture on which it is easy to detect minute differences in pattern. On the other hand we would remind the conchologist that the external parts are by their very positions most liable to exhibit lesions and weathering, and certainly in the case of Mollusca where the dependence of the exoskeleton upon a specific article of diet (viz., lime salts) is very close, to register "fluctuating variation" according to the constitution of the medium or of the food ingested.

But here again if a more common practise were made in dissecting by systematists, variations would be found even in closely allied species making the descriptions complete and often even necessitating the erection of new genera. One of the writers was dissecting an ordinary species when he discovered that the epipharynx was so entirely different in form and structure from the usual type for the genus that, had it been an external character it would long ago have been formed into a new genus.

Karel Thon<sup>1</sup> has demonstrated how in *Holothyridae* a single internal structure is at variance with the other indications of genetic affinity. A great many similar instances will be immediately called to mind by those who practise dissection.

Again, if systematists are convinced of the taxonomic value of hard parts how comes it that they need to be reminded that there are hard parts in the internal anatomy as well which they so frequently and habitually leave unnoticed? The endoskeleton of Arthropods, gastric mills, pharyngeal ossicles and cartilaginous supports are all systems which might be profitably studied by the entomologist and carcinologist, while the conchologist generally proceeds as though the radula and jaw were part of the "mush," as he so inelegantly terms the viscera.

#### GEOGRAPHICAL DISTRIBUTION

The advent of the morphologist into the particular sphere of systematics or the metamorphosis of the systematist into a morphologist (it matters not how we put this desirable event) will result in the annexation not only of classification, but also of questions of geographical distribution by anatomy and morphology. How many pretty theories in geographical distribution

<sup>1</sup> *Zool. Jahrb.*, Bd. XXIII, Syst., pp. 720-21.

have collapsed because they were built on the sands of an incorrect classification? The similarity between the faunas of South America and Madagascar is supported by many facts, but the value of *Solenodon* in Cuba and *Centetes* in Madagascar has been lessened by the recognition that the two genera resemble each other by convergence, and should now be classified in different families.

The Dendrobatinae also are considered by Dr. Gadow as an unnatural group, the two divisions—South American and Mascarene—having, according to him, lost their teeth independently. Again, Dr. Gadow refers to the Ratitae as a heterogeneous assemblage of birds which is “absolutely worthless” for the zoogeographer. There are scores of such artificial groupings—the work of the systematist—which have led zoogeographers astray.

The result is that systematic work as at present pursued is of very little use to us in the study of geographical distribution. It is hopeless nowadays for a zoologist to sit down with a list of species and their range and trusting implicitly in systematic work to make maps of distribution and, as he so often does, to draw deductions therefrom, for the validity of such deductions must ultimately depend upon the anatomical and morphological data. Moreover the study of geographical distribution is developing new methods of tackling its problems.

We do not consider it necessary to touch on the other remedies that might be applied with a view to redeeming zoological taxonomy from its present artificial state and to bringing it into line with the rest of biology.

Such remedies—for instance, testing the validity of species by genetic experiment and the intensive study of variation—have been advocated many times before,<sup>2</sup> although with little success. We believe, however, that the reforms in descriptive zoology we have advocated above are the more urgent.

<sup>2</sup> Cf. E. B. Poulton, “Essays on Evolution,” 2. “What is a Species?” and K. Jordan, “Novitates Zoologicae,” 3, 1896.